USPS–RT–6

BEFORE THE
POSTAL RATE COMMISSION
WASHINGTON, D. C. 20268-0001

POSTAL RATE AND FEE CHANGES, 2000

Docket No. R2000–1

REBUTTAL TESTIMONY
OF
A. THOMAS BOZZO
ON BEHALF OF THE
UNITED STATES POSTAL SERVICE
(CONCERNING MAIL PROCESSING VOLUME-VARIABILITY)

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Autobiographical Sketch

My name is A. Thomas Bozzo. I am a Senior Economist with Christensen Associates, an economic research and consulting firm located in Madison, Wisconsin. My education and experience are described in detail in my direct testimony, USPS–T–15.

I. Purpose and Scope of Testimony.

The purpose of this testimony is to rebut criticisms of the Postal Service's econometric estimates of volume-variability factors for mail processing labor, and of the underlying economic theory and econometric methods, found in the testimonies of witnesses Neels (UPS–T–1) and Smith (OCA–T–4).

Associated with my testimony is Library Reference LR–I–457, which contains the background material for the analyses reported in this testimony. The accompanying CD-ROM contains electronic versions of the spreadsheets and programs used for the analyses presented herein.

II. Dr. Neels's aggregate time-series models yield Cost Segment 3.1 variabilities well below 100 percent when obvious flaws are corrected.

In this section of my testimony, I review Dr. Neels's aggregate time-series analysis, which he represents as "a conceptually superior alternative to the MODS-level analysis presented by Dr. Bozzo." Tr. 27/12835. As Dr. Greene indicates, Dr. Neels's conclusion that his aggregate time series model is "conceptually superior" is erroneous. USPS-RT-7 at 5. Among other flaws noted by Dr. Greene, Dr. Neels's aggregate time series model imposes a variety of restrictions on the response of costs to technological change and to variabilities at the site and activity levels which are not warranted *a priori*. Dr. Neels's analysis also discards most of the information in the underlying micro data. I concur with Dr. Greene, and by way of addition, note that Dr. Neels's time series analysis is materially identical to the simple regression models that the Postal

Service rejected as a basis for variabilities more than thirty years ago. A number of deficiencies of Dr. Neels's approach are already described in some detail in my direct testimony. USPS–T–15 at 9-12. Chief among these, as was noted by the Cost System Task Force back in the late 1960s, is the inability to identify and control for the effects of non-volume cost-causing factors. USPS–T–15 at 11.

Below I show that, notwithstanding the fundamental conceptual errors in his aggregate time series approach, Dr. Neels's quantitative results—estimates of cost segment 3.1 "volume variability" ranging from 98 percent to 123 percent in his Table 11 (Tr. 27/12840), and 109 percent to 119 percent in his Table 12 (Tr. 27/12842)—are artifacts of errors he committed when building his model. When these flaws are corrected, his models produce aggregate volume-variability estimates for Cost Segment 3.1 that are significantly less than 100 percent, results generally consistent with the results from my disaggregated models.

When performing aggregate time-series regression analysis, it is essential that the data used for estimation consist of observations on variables that are consistently defined throughout the sample period. If not, the analysis is effectively comparing apples and oranges, and produces nonsensical results.[1]

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[1] In time-series modeling the data are regarded as a single realization from an underlying data generating process that governs the values of the variables in each period. If the definition of a variable changes materially within the sample period, the process generating the observations that occur prior to the change cannot be said to apply to those that occur after it. It is therefore incumbent upon the analyst to either correct the data or incorporate the changed definition explicitly into his model. See, e.g., A.C. Harvey, *The Econometric Analysis of Time Series*, Phillip Allan 1981, at 14 et seq.

Dr. Neels estimates several variations on his time-series model using "aggregate, system-level [annual] time series data on volumes and mail processing [labor] costs." Tr. 27/12835. "The mail processing costs data for cost segments 3.1 (Mail Processing Clerks and Handlers), 2.1 (Mail Processing Supervisors), and 11.2 (Mail Processing Operating Equipment Maintenance) [were] taken from the Postal Service's response to Interrogatory UPS/USPS-T11-7-17, Tr. 21/9351-52." Tr. 27/12836. Dr. Neels's first error was failing to account for changes to the definition of Cost Segment 3.1 that occur during the sample period even though he is aware of these changes:

> I have reviewed the documentation on changes in the definition of Cost Segment 3.1 cited by the Postal Service in response to UPS/USPS-T11-8. Several changes in the definition have occurred. Because they do not appear to be of a significant nature, *I have not accounted explicitly for these changes.* Response to USPS/UPS-T1-14, Tr. 27/12940 (emphasis added).

In fact, Dr. Neels makes no effort to account for changes in the definition of Cost Segment 3.1 whatsoever. Furthermore, Dr. Neels was wrong to suppose that the definition of Cost Segment 3.1 does not change significantly during the sample period. In his data set, FY97 and FY98 Cost Segment 3.1 costs include the so-called "migrated" costs from Cost Segments 3.2 and 3.3, whereas the remaining cost observations do not. The implications for the measured segment 3.1 costs are not trivial. FY97 and FY98 segment 3.1 costs in the Postal Service's methodology are, respectively, $801 million and $570 million greater than the corresponding totals from the Commission's methodology, which continues the pre-Docket No. R97-1 definition. It is interesting that he should characterize the

change as "not... of a significant nature" since another UPS witness (witness Sellick) has, ostensibly in response to Dr. Neels's advocacy of the 100 percent variability assumption, opposed the redefinition of segment 3.1 in this proceeding and in Docket No. R97-1. Tr. 27/13126. It is all the more ironic as Dr. Neels has made something of a career out of criticizing Postal Service witnesses who, in his view, fail to adequately scrutinize their data sets.[2] In this case, Dr. Neels fails to perform even a modicum of quantitative analysis to justify his assumption that the changes to Cost Segment 3.1 were "not...of a significant nature." Response to USPS/UPS-T1-48(a) at Tr. 27/13009.

To correct Dr. Neels's mistake, I reran his aggregate time series regressions using a consistent definition of Cost Segment 3.1 costs. Since recasting years prior to FY96 using the Postal Service's Docket No. R97-1 method is difficult, I chose to use the PRC's definition of Cost Segment 3.1 as explained in the Docket No. R97-1 Opinion. PRC Op. R97-1, Vol. 1 at 93-95, 117-118, 126. As I show in Table 1, when a clean cost series is used, Dr. Neels's time series analysis produces lower variabilities than those he originally reported based on the inconsistently defined series.

A second error in Dr. Neels's analysis concerns the exclusion of FY79 and FY80 observations from his time series analysis. He excluded those observations because he claims there is uncertainty as to whether zero reported volumes for First-Class carrier route presort and Third Class 5-digit presort

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[2] In the present docket see, e.g., Tr. 27/12792, 12796-12802; in Docket No. R97-1 see, e.g., Tr. 28/15590-91, 15600-609, 15799-800.

represent "true zeroes" or reporting errors. Response to USPS/UPS–T1–47(d) at Tr. 27/13007. Dr. Neels's error in this instance is one of omission rather than commission. The rate history information provided in USPS-LR-I-118 clearly shows that the rate categories in question did not exist until FY81. Witness Fronk's testimony also references the FY81 introduction of carrier route presort discounts for First-Class Mail. USPS-T-33 at 13. Including the FY79 and FY80 observations in the time series regressions lowers the estimated variabilities by a few points.

The third, and most quantitatively significant, error in Dr. Neels's time series analysis is the underspecification of his model. Dr. Neels freely combines data from the Postal Service's automation and pre-automation eras, and neglects to include any variables to capture the effects of such patently non-volume factors as the network served by the Postal Service. Dr. Neels's justifications for this approach, that his omissions capture a truer picture of the effect of volume on costs, and that there are no likely omitted non-volume factors (Tr. 27/12938-9), are unsupportable on operational and statistical grounds. Omitting relevant variables from a regression leads to bias. Dr. Neels's own model does not follow what he himself calls "basic econometrics." Tr. 27/12939. Furthermore, Dr. Neels concedes elsewhere in his direct testimony that serving its network is costly to the Postal Service, so the argument that non-volume factors that affect costs do not exist strains credulity. Dr. Neels should have employed a more richly specified model.

One way of exploring the effects of the specification error is to split Dr. Neels's sample and reestimate his model. I have done this, and report the results below in Table 1. Splitting the sample has the effect of relaxing the assumption of Dr. Neels's time series model that the same cost relationship applies to all time periods, irrespective of the extent of the network served, the technology employed, and other factors. An obvious choice of the split point is between the period covered by the Postal Service's variability studies (FY88-FY98) and the previous period. This analysis allows for a better apples-to-apples comparison of results between Dr. Neels's time series models and the Postal Service's studies in my testimony and that of Dr. Bradley in Docket No. R97-1. The results from the split sample are remarkably different from those reported by Dr. Neels. The estimated variabilities obtained using the FY88-98 observations range from 67.5 to 84.8 percent, depending on the choice of worksharing parameter. These results are broadly consistent with the Postal Service's disaggregated models.

Dr. Neels expresses concern that there were too few observations to reliably estimate the variabilities in defending his failure to estimate his models over the time period studied by Dr. Bradley and myself. Tr. 27/13060. My analysis shows that this concern is unfounded, however, as the standard errors of the variabilities from this shorter time period are only a couple of percentage points higher than those obtained from the larger sample. The estimated variabilities using the FY88-FY98 observations are lower than 100 percent by a statistically significant amount. Nor is it the case that fitting the time series model

to the earlier observations shows that the pre-FY88 variabilities exceed 100 percent. There, too, the variability estimates are somewhat less than 100 percent.[3]

However, the purpose of this analysis is not to try to rehabilitate the aggregate time series analysis. Rather, it is simply to demonstrate that, when cast on an apples-to-apples basis, and using minimally appropriate data, the time series analysis fails to demonstrate 100 percent variability.

A final point concerns the nonlinear least squares model that Dr. Neels employs to validate the choice of worksharing parameter. While the variability estimate from this analysis is notably high—119 percent—the standard error of the estimate, 0.3, is also extremely high. As a result, not only is the 119 percent variability not significantly different from 100 percent, but at a 90 percent confidence level it is not statistically different from 70 percent. The standard error of the worksharing parameter estimate is also very large. The estimated value of 0.855 is not significantly different from any of the estimates Dr. Neels used for the analysis presented in Table 12 of UPS–T–1. Tr. 27/13064.

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[3] The high standard errors suggest that the simple time series model does a poor job of explaining segment 3.1 costs in the FY79-FY87 period. Again, this suggests the need for a more richly specified regression model than Dr. Neels's aggregate time series approach.

Dr.Neels's nonlinear least squares results are rendered useless by the high standard errors of the estimates.

Table 1.
Sensitivity of Dr. Neels's Time Series Analysis to Modeling Choices:
Estimated "Volume Variabilities" (Standard errors in parentheses)

Model	Neels, UPS-T-1 Table 11	FY79-FY98 Observations, Neels Data	FY79-FY98 Observations, Consistent Data	FY88-FY98 Observations, Consistent Data	FY79-FY87 Observations, Consistent Data
Worksharing parameter = 0.6	.979 (.068)	.930 (.057)	.880 (.053)	.675 (.076)	.781 (.189)
Worksharing parameter = 0.7	1.048 (.073)	1.001 (.061)	.948 (.056)	.748 (.079)	.843 (.199)
Worksharing parameter = 0.8	1.135 (.078)	1.092 (.065)	1.035 (.059)	.848 (.082)	.919 (.212)

Sources: Tr. 27/12840; USPS-LR-I-457.

III. Correcting obvious flaws in Dr. Neels's analysis of the relationship between TPH and FHP yields the operationally plausible result that the elasticity of TPH with respect to FHP is approximately unity, which supports my methodology.

In this section of my testimony, I review Dr. Neels's analysis of the relationship between TPH and FHP.[4] First, I discredit Dr. Neels's claim that I used TPH as an erroneous "proxy" for mail volume, an argument that was also refuted by Dr. Christensen in Docket No. R97-1. Then, I refute Dr. Neels's "reverse" regression analysis: the analysis itself is mishandled sufficiently that the results are meaningless; but even if he had not made hash of the analysis, Dr. Neels clearly has failed to grasp its meaning. Finally, the available evidence, while not conclusive, generally supports the result that the elasticity of TPH with respect to FHP is approximately unity, thereby supporting my methodology.

As he did in his R97-1 testimony, Dr. Neels continues to promote the canard that using piece handlings to estimate volume-variability factors for MODS mail processing labor costs constitutes an erroneous reliance on "a proxy for true [sic] volume." Tr. 27/12791-93, 12802; see also Docket No. R97-1, Tr. 28/15594-600. Under this theory, Neels seeks to estimate the elasticity of TPH with respect to FHP (that is, $\partial \ln(TPH)/\partial \ln(FHP)$) in order to "correct" my volume-variability estimates by a multiplicative factor. Tr. 27/12832; Tr. 27/12902-3.

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[4] In this section of my testimony, "TPH" should be read as "TPF or TPH, as appropriate."

The "volume proxy" issue is a red herring because, as Dr. Neels himself concedes in his testimony, I do not use piece handlings as a proxy for subclass volumes, but rather as an intermediate cost driver. Tr. 27/12802; see also USPS-T-15 at 52-53. Under the "cost driver/distribution key" (or, for short, "distribution key") approach to measuring volume-variable costs in mail processing, piece handlings are taken to be the "outputs" (cost drivers) of mail processing operations, not proxies for volume. The volume-variability factors, which are elasticities of hours with respect to piece handlings in an operation, are combined with distribution keys, which are estimates of the elasticities of piece handlings with respect to subclass (RPW) volumes, to form the elasticities of hours with respect to subclass volumes. USPS-T-15 at 52-56. The distribution key approach constitutes a feasible approach for estimating subclass volume-variable (or, when unitized, marginal) costs because it decomposes the relationship between cost and RPW volume, which cannot be directly estimated, into components that can be estimated. As I discuss in more detail below, the distribution key method is an economically appropriate method to estimate volume-variable costs for rate making.

Dr. Neels is unjustifiably selective in criticizing the application of the distribution key approach to mail processing costs. He finds that the distribution key approach is a reasonable method of measuring volume-variable costs in some contexts—he specifically mentions its use in analyzing Cost Segment 14, purchased highway transportation. Tr. 27/12802; Tr. 27/12999. However, he claims that it should not be used to analyze mail processing costs. Tr. 27/12804.

Dr. Neels is clearly inconsistent on this point: does he claim that cubic foot-miles, the cost driver in Cost Segment 14, is a valid "proxy for delivered volume"? Of course not: it is obviously not that, nor need it be. It is merely a cost driver, as is piece handlings.

Dr. Neels testifies that there are two key assumptions underlying the cost driver/distribution key methodology: the first is "that the cost driver captures the essential cost-causing characteristics of the various subclasses." Tr. 27/12802. The second "is that the cost driver changes in direct proportion to the volume of mail" – the so-called "proportionality" assumption. Tr. 27/12803. Regarding the first assumption, Neels offers no supportable objection to my argument that piece handlings is a valid cost driver in mail processing operations. Instead, he raises the red herring that piece handlings are a poor proxy for delivered mail volume. Tr. 27/12803. As I argued above, this feint is clearly an attempt to distract, since Neels knows that whether or not TPH is a good "proxy" for delivered mail volume is irrelevant and has no bearing on the necessity of estimating elasticities with respect to piece handlings. Dr. Neels's "corrections" are at best superfluous, and should be rejected. Nor is it a requirement of the distribution key approach that there be a single cost driver that captures all relevant characteristics. As Dr. Christensen demonstrated in Docket No. R97-1, the distribution key method can readily be generalized to accommodate multiple cost drivers. Docket No. R97-1, USPS–RT–7 at 6-7, Tr. 34/18222-3.

Nonetheless, without conceding the relevance of Dr. Neels's FHP-TPH analysis or the validity of the "corrections" he derives from it, his analysis of the

statistical relationship should be examined, since virtually every aspect of his analysis seems conceived to misstate or obfuscate the true relationship between TPH and FHP, let alone TPH and RPW volume. Dr. Neels attempts to investigate the statistical relationship between TPH and FHP "as a test of the 'proportionality assumption'" between piece handlings and mail volume. Response to USPS/UPS–T1–3(a) at Tr. 27/12899. However, the proportionality assumption concerns the relationship between TPH and RPW volume, not TPH and FHP volume. Dr. Neels's analysis, at best, simply substitutes one proportionality assumption for another—to be dispositive of the proportionality assumption for TPH and RPW volume, Dr. Neels's FHP analysis must assume proportionality of FHP and RPW volume. Tr. 27/13046-7. Furthermore, citing the Docket No. R97-1 bogeyman of FHP measurement error, he chooses a statistical method—reverse regression—for estimating the TPH-FHP relationship that, for reasons Dr. Greene discusses at some length in USPS-RT-7 at 23-24, would be expected to produce an upwardly biased result. Needless to say, an upwardly biased estimator makes it much easier for Dr. Neels to demonstrate the need for a disproportionality "correction" to the Postal Service's variabilities.

The FHP measurement error motivation for the reverse regression estimator is extremely weak. As Dr. Greene indicates, measurement error needs to be quite severe before even trivial attenuation of "direct" regression estimates would be expected to occur in the classic errors-in-variables model. USPS–RT–7 at 24-26. Accordingly, Dr. Neels should have at least tried to estimate the direct regression equation. But he did not estimate, or even specify, the direct

regression he purported to estimate. The surprising—and operationally implausible—result of "disproportionate increases in piece handlings [TPH or TPF]" (Tr. 27/12805) in response to an increase in FHP volume should be rejected as the erroneous progeny of Dr. Neels's inappropriate estimation procedures.

In what follows, I review Dr. Neels's handling of the problem of estimating the statistical relationship between FHP and TPH, highlighting the major errors he committed. Then I show that when these errors are corrected, the evidence supports the conclusion that the elasticity of TPH with respect to FHP is approximately unity.

Dr. Neels has testified that his purpose in performing his "reverse" regression analysis was to "estimate the elasticity of TPH/F with respect to FHP." Tr. 27/12806. In other words, he wanted to obtain consistent estimates of the parameters of the function relating TPH to FHP (and other relevant variables) and then use them to compute the elasticity, which is a function of the parameters. This implies that he had a model in mind of the regression function relating TPH to FHP and other relevant variables. However, Neels chose not to work with the direct regression of TPH on FHP because he believes that FHP is an error-ridden proxy for volume:

> FHP is known to be a very noisy measure of volume....To avoid the pitfalls of errors-in-variables bias, I estimated the elasticity of TPH/F with respect to FHP using the reverse regression of FHP on TPH/F and other variables....The reverse regression isolates the mismeasured variable FHP as the dependent variable. Tr. 27/12805-6.

Moreover, Neels never explicitly specified this "forward" or "direct" regression model, either in his testimony or in his interrogatory responses. Indeed, he twice refused direct requests to specify what it looked like. Tr. 27/12968, 13015-6. This refusal is telling: were he to have explicitly specified the forward model corresponding to his so-called "reverse" regression model, it would have made clear that his "reverse" regression specification was nonsensical.

Proper econometric practice demands that the analyst explicitly specify the forward regression model of interest, and then derive the reverse regression specification from it – this is the only way to know that the parameter or elasticity estimate obtained from the reverse regression bears any meaningful relationship to the desired statistic from the associated forward regression. If the reverse regression is specified in an ad hoc fashion, one runs the risk of seriously misspecifying the direct regression, which would then yield meaningless results.

This point is important because Dr. Neels claims to have derived an admissible estimate of an elasticity that would be appropriately defined in terms of the direct relationship between TPH and FHP without even specifying the relationship. Indeed, he specified his reverse regression in such a way that he is unable to say what the forward regression function looks like. Tr. 27/12968. He argues that the direct regression equation can only be defined implicitly (Tr. 27/12968), and provides some analysis that purports to show that his reverse regression elasticity formula is appropriate. His argument is entirely circular—change the specification of the reverse regression, and the result Dr. Neels reports at Tr. 27/12802 changes. See also Tr. 27/13055-6. The only logical

conclusion for his arguments is that they allow him to deflect attention away from the fact that one could easily specify, and estimate with reasonable accuracy, the direct regression relationship between TPH and FHP.

Let us therefore ask the basic question that Neels himself should have asked, but apparently never did: what is the relationship between TPH and FHP? Ironically, the information needed to specify a reasonable forward model is contained in Dr. Neels's own testimony and interrogatory responses. "A single piece of mail...will generate a unit increase in FHP volume at each of the processing plants through which it passes and in which it undergoes sortation." Tr. 27/12900. Continuing, "A piece handling, however, is generated each time a piece of mail at a specific site is processed in a particular sorting activity." Tr. 27/12803. Therefore, for a given site, the following identity holds:

(2) $TPH_{it} \equiv FHP_{it} \cdot HPP_{it}$

where HPP_{it} is the average handlings per piece for a given plant and time period. This identity expresses the truism that the total piece handlings in an operation (for a given plant and period) is the product of the number of pieces initially entering the operation and the number of handlings the average piece receives in that operation. This, then, is the fundamental relationship between TPH and FHP.

In logarithms, equation (2) is:

(3) $\ln TPH_{it} = \ln HPP_{it} + \ln FHP_{it}$.

From equation (3), it follows immediately that if handlings per piece are constant with respect to a change in FHP volume, there is "100 percent variability" of TPH with respect to FHP, that is:

(4) $\partial \ln TPH_{it} / \partial \ln FHP_{it} = 1.$

Equation (4) demonstrates that Dr. Neels's results require that handlings per piece must increase with volume, or:

$\partial \ln HPP_{it} / \partial \ln FHP_{it} > 0.$

To flesh this relationship out for statistical analysis, we need to expand the HPP term by understanding that it is a function of other variables, potentially including FHP. Additionally, HPP would be expected to depend on network characteristics, and a trend should be included to account for technical changes and other trend factors not elsewhere specified in the model. Therefore we can rewrite equation (2) as the following general function:

(5) $TPH_{it} \equiv FHP_{it} \cdot HPP(FHP_{it}, SITE_t, NETWORK_t, PERIOD_t)$

where HPP(·) indicates the function defining HPP. Discussion of the precise form and content of the SITE, NETWORK, and PERIOD terms is postponed for the moment. This equation is intended to apply at the shape level. At the operation level, it would be necessary to further complicate the relationship in order to relate TPH at the operation to FHP in all upstream operations where a given piece might have received its first distribution handling.

Taking natural logarithms of both sides of equation (5) yields:

(6) $$\ln TPH_{it} = \ln FHP_{it} + \ln[HPP(FHP_{it}, SITE_t, NETWORK_t, PERIOD_t)]$$

Equation (6) says that the logarithm of TPH is an unknown function of the logarithm of FHP as well as site and network characteristics and time period. Since the form of this function is unknown, current best econometric practice dictates that a fully flexible functional form (including interaction terms, which Dr. Neels inexplicably dropped from his regressions), with site fixed effects and either quarter dummies or a time trend, is the preferred specification for empirical work. I chose the translog form to expand the expression for ln(HPP), and the resulting direct estimating equation remarkably resembles the equation that Dr. Neels could not confirm represented the direct equation corresponding to his reverse regression. Response to USPS/UPS–T1–33(d) at Tr. 27/12968. The translog version of the direct regression model relating TPH to FHP is:

(7) $$\begin{aligned}\ln(TPH_{it}) = {} & \gamma_i + \gamma_1 \ln(FHP_{it}) + \gamma_{11}[\ln(FHP_{it})]^2 + \gamma_2 \ln(DPT_{it}) + \gamma_{22}[\ln(DPT_{it})]^2 \\ & + \gamma_3 t + \gamma_{33} t^2 \\ & + \gamma_{12}[\ln(FHP_{it}) \cdot \ln(DPT_{it})] + \gamma_{13}[\ln(FHP_{it}) \cdot t] \\ & + \gamma_{23}[\ln(DPT_{it}) \cdot t] + \mu_{it}\end{aligned}$$

where DPT is delivery points, t is a time trend, and μ is the direct regression disturbance.[5] The relevant elasticity from the direct regression is the marginal effect of FHP volume processed at a plant on the number of piece handlings at that plant:

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[5] Note that equation (7) includes a time trend rather than individual quarter dummies as Dr. Neels's model does. This was done primarily to simplify the

(8) $$\partial \ln(TPH)/\partial \ln(FHP) = \gamma_1 + 2\gamma_{11}\ln(FHP) + \gamma_{12}\ln(DPT) + \gamma_{13}t\,.$$

In the results I report below, I evaluate the elasticity formula given by equation (8) at the arithmetic sample mean values of the variables on the righthand side.

When equation (8) is compared with the expression that Dr. Neels derived from his reverse regression model using the implicit function theorem (Response to USPS/UPS-T1-52 at Tr. 27/13015), it is clear that they are quite different:

(9) $$\frac{d\ln(TPH)}{d\ln(FHP)} = \frac{1}{\beta_1 + 2\beta_2\ln(TPH)} \neq \gamma_1 + 2\gamma_{11}\ln(FHP) + \gamma_{12}\ln(DPT) + \gamma_{13}t$$

Note in particular that Dr. Neels's elasticity – the middle term in (9) – is a function of TPH, while the correct elasticity is a function of FHP and DPT. His claim that his result "is exactly the inverse of the marginal effect of TPH on FHP from the regression of FHP on TPH...presented in UPS-T-1" (Id.) may be true as a matter of purely abstract reasoning. But it obviously *is not the relevant elasticity derived from the correctly specified forward model shown in equations (7) and (8).* The obvious asymmetry between the elasticity derived from the direct regression and that which Dr. Neels derives from his reverse regression helps explain Dr. Neels's erroneous results.

Even ignoring the lack of correspondence between Dr. Neels's reverse regression specification and the properly specified forward regression shown in equation (7), as Dr. Greene describes in his testimony, Dr. Neels cannot claim

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specification of interactions between time and the other variables and should not be construed as a criticism of the time dummy approach, per se.

that his reverse regression result provides a consistent estimate of the elasticity he is seeking. The most he could reasonably claim to have found with his reverse regression estimates is an upper bound for the true (unknown) value. Tellingly, Neels makes no claims, in testimony or interrogatory responses, about the consistency or unbiasedness of his TPH/FHP elasticities. As Dr. Greene argues, this alone is good reason why Neels's "reverse" regression analysis, and the results in his Tables 6 and 7 that depend on it, should be rejected. USPS–RT–7 at 34-35.

I estimated equation (7) and the elasticities defined in equation (8) for the combined letter and flat shape operations, using the data I provided in LR-I-107 and LR-I-186. I employed the same panel data fixed effects estimator that Dr. Neels used, but did not impose an adjustment for AR(1) disturbances. The omission of the autocorrelation adjustment simplifies the programming somewhat; it does not bias the results. I report my results in Table 2. I did not attempt to estimate elasticities at the cost pool-level. To appropriately do so, as I stated above, it would be necessary to greatly complicate the TPH-FHP models to account for the fact that TPH in one cost pool may, and often will, appear as FHP in another cost pool.

The results in Table 2 contrast sharply with those presented by Dr. Neels. The direct regressions for the letter and flat shapes produce TPH-FHP elasticities between 0.92 and 0.95 for letters, and approximately 0.81 for flats, depending on which observations are used to evaluate the elasticity functions. These results cannot, however, be used as evidence on the proportionality assumption—the

decisive data for that purpose would be the elasticities of FHP with respect to subclass RPW volume, which cannot be estimated given the limited RPW volume data available.

What these data do suggest, however, is that the TPH-FHP relationship is not likely to be grossly different from a 100 percent variability relationship. There is no reason why Dr. Neels's misconceived reverse regression model should produce a reasonable upper bound on the TPH-FHP elasticity. Furthermore, the direct regression results, combined with Dr. Greene's theoretical exposition, strongly suggest that the true value of the elasticities are close to the direct regression results. Dr. Greene observes that an effect of measurement error would be to "bias the fit of the model downward." USPS-RT-7 at 25. But the direct TPH-FHP regression models, like many others based on my data set, exhibit very high values of the R^2 statistic. The FHP, generated through weight conversions, do an excellent job of explaining the variation in the mainly machine-counted TPH and TPF. The FHP data could not do so if they exhibited extreme measurement error of the sort Dr. Neels assumes. The evidence suggests that measurement error is not likely to be a major problem. Of course, without material measurement error, Dr. Neels's pretense for employing the reverse regression technique evaporates.

In summary, the evidence Dr. Neels provides purporting to overturn the "proportionality assumption" does nothing of the sort. Dr. Neels employed an inappropriate estimation method to produce a nonsolution to a nonproblem. The Commission should reject his analysis.

Table 2.
Direct regression estimates of TPH-FHP elasticities[1]

Shape	Letters	Flats
TPH-FHP Elasticity (evaluated with all observations)	0.950 (.015)	0.811 (.008)
TPH-FHP Elasticity (evaluated with FY98 observations)	0.920 (.016)	0.813 (.009)
Adjusted R-squared	0.991	0.995
Number of observations	5,603	4,980
Number of sites	303	276

[1]Elasticities evaluated using arithmetic mean method; standard errors in parentheses.

IV. Dr. Neels's shapes level models, though likely to be biased, support the conclusion that variabilities for mail processing operations are less than 100 percent.

Dr. Neels attempts to improve on the cost pool-level models of the relationship between hours and TPH/F by estimating models on data aggregated to the shapes level. Ostensibly, the purpose of the aggregation is to capture the effects of interactions among operations that Dr. Neels contends are ignored in the cost pool-level models, and to overcome supposed data errors along the lines of the "commingling" of manual parcel and SPBS data that Dr. Neels erroneously believes to occur. Tr. 27/12829.

However, Dr. Neels's shapes-level models fail to establish any indication of bias in the cost pool-level results. I reproduce Dr. Neels's shapes-level results, along with the corresponding cost pool-level results from USPS-T-15, in Table 3 below. For Dr. Neels, the would-be smoking gun appears to be the

result that his flat and parcel shape models yield higher elasticities than the corresponding cost pool level models. Tr. 27/12829-30. However, as Dr. Neels notes, the letter shape model yields a variability estimate over 17 percent lower than that which results from the cost pool model. Tr. 27/12831. Insofar as the letter shape cost pools are much larger than the combined flat and parcel shape cost pools, and the shape-level elasticity for flats is only approximately 7 percent higher than the composite cost pool-level value, the net effect of the aggregation to shape level is a composite variability of 73.1 percent for the pools covered by Dr. Neels's analysis—7 percent lower than the 78.6 percent composite that results from my cost pool-level models. Following Dr. Neels's logic, if my cost pool-level results are biased because of the interactions of operations and supposed data errors that motivate the shapes-level analysis, the net effect is actually a slight upward bias. Furthermore, even the higher flat and parcel shape elasticities estimated by Dr. Neels are still significantly lower than 100 percent, as is the 66.3 percent letter shape level variability. However, Dr. Neels's logic that the differences between the shape level and cost pool level models reflect biases in the cost pool level models is wrong. As with the aggregate time series and group means ("between") regressions, the problem is aggregation. The shapes level models are simply aggregates of the cost pool models. Tr. 27/12829. As Dr. Greene notes, aggregation imposes restrictions on the shape level models that are not present in the cost pool models. Then, if the restrictions of the shape level models are correct, and the disaggregation by cost pool really does not add anything to the model, the cost pool and the shapes level models should produce

the same results, at least statistically. But, as Dr. Neels points out, they do not. Tr. 27/12830. The correct conclusion to draw is that the shape level models impose inappropriate restrictions and that the results reported by Dr. Neels in Table 8 (Tr. 27/12832) are biased. Dr. Neels's interpretation is the opposite of the statistically correct conclusion and must be rejected.

As a final note, Dr. Neels's shape level analysis marks a major change from his Docket No. R97-1 testimony. In Docket No. R97-1, Dr. Neels argued, based on the results of the group means regression or "between" model, that it was not possible to exclude on statistical grounds the possibility that the TPH elasticities were equal to or greater than 100 percent. As I explain in my direct testimony—and Dr. Greene further explains in USPS–RT–7— Dr. Neels's assertions that the group means model is appropriate (and Dr. Smith's claim that the group means regression is "least bad") are based upon badly flawed statistical logic. USPS–T–15 at 122-124 and USPS–RT–7 at 30-31. In his current testimony, one statistical error Dr. Neels does *not* make is to attempt to rehabilitate his previous recommendation of the "between" model's results. The end result is that no econometrically defensible result on the record of this proceeding suggests anything other than that the elasticities of hours with respect to TPH are less than 100.

Table 3.
Effect on BY98 Volume-Variable Costs of Substituting Neels Shape-Level Variabilities
(without FHP adjustment) for Postal Service Variabilities

	(1) Pool Total Cost, BY98 ($000)	(2) Neels Variability	(3) Bozzo Variability	(4) Neels Shapes	(5) Neels Pool Variable Cost, ($000)	(6) Pool Variable Cost ($000)	(7) Neels Shape Variable Costs ($000)	(8) Difference ($000)	(9) Percent Difference
Letter Shape Cost Pools									
BCS	1,043,841	0.897	0.895		936,325	934,238			
LSM	78,765	0.956	0.954		75,299	75,142			
Manual Letters	1,563,964	0.737	0.735		1,152,641	1,149,514			
OCR	219,070	0.752	0.751		164,741	164,522			
Subtotal	2,905,640			0.663	2,329,007	2,323,415	1,926,439	-396,975	-17.1%
Flat Shape Cost Pools									
FSM	1,042,369	0.82	0.817		854,743	851,615			
Manual Flat	459,933	0.773	0.772		355,528	355,068			
Subtotal	1,502,302			0.857	1,210,271	1,206,684	1,287,473	80,789	6.7%
Parcel Shape Cost Pools									
Manual Parcel	60,593	0.522	0.522		31,630	31,630			
SPBS Non-Priority	283,275	0.645	0.653		182,712	184,979			
SPBS Priority	82,446	0.645	0.653		53,178	53,837			
Subtotal	426,314			0.75	267,520	270,445	319,736	49,290	18.2%
Total	4,834,256				3,806,797	3,800,544	3,533,648	-266,896	-7.0%
Composite[1]					78.7%	78.6%	73.1%		

[1]Composite is volume-variable cost as a percent of pool total cost for all reported pools.

Sources:
(1) USPS–T–17, Table 1
(2) Tr. 15/6386
(3) USPS–T–17, Table 1
(4) UPS–T–1, Table 8
(5) (1) * (2)
[6] USPST17, Table 1
[7] (4) * (1)
[8] (7) * (6)
[9] (8) * (6)

V. Dr. Neels's criticisms of the "distribution key" method, not to mention MODS cost pools, are fundamentally at odds with the findings of the Data Quality Study, and are especially ironic as the UPS mail processing cost method is transparently an application of the "distribution key" approach with 100 percent variabilities.

In this section of my testimony, I revisit Dr. Neels's criticisms of the "cost driver/distribution key" method of measuring volume-variable costs, as described in my testimony, in light of the findings of the Data Quality Study and the testimony of UPS witness Sellick. In his direct testimony Dr. Neels states that "[I]t would be even simpler for the Postal Service to dispense with the whole cost driver/distribution key approach and retain the traditional finding that mail processing labor costs are 100 percent volume variable." Tr. 27/12804. Elsewhere, he criticized my decision to "base [my] analysis on each MODS cost pool in isolation" rather than working with more highly aggregated data. Tr. 27/12793. These views put him squarely at odds with the conclusions of the recent Data Quality Study, jointly sponsored by the Postal Service, the GAO, and the Commission and, ironically, also with UPS witness Sellick, whose mail processing cost proposal is transparently an alternative application of the Postal Service's distribution key methodology using 100 percent variabilities. Response to USPS/UPS–T2–1 at Tr. 27/13133.

The authors of the Data Quality Study are generally quite favorably disposed towards the cost driver/distribution key approach. Moreover, they do not support the continued assumption of 100 percent volume variabilities for mail processing. For instance, in the section discussing cost attribution, they state

that measuring volume-variability factors as the proportional change in a cost pool with respect to a unit proportional change in a cost driver, far from being the suspect practice that Neels would have us believe, is in fact "logical" and "correct":

> The main economic issues arising from data quality problems in the determination of attributable costs are in the modeling of cost elasticities (or "volume variability factors" using Postal Service terminology). As noted in the VVC equation above, these cost elasticities are intended to measure the percentage change in an accrued cost pool in response to a given percentage change in the Cost Driver of the respective pool. *Logically, this is the correct approach.* (*Technical Report #1: Economic Analysis of Data Quality Issues* at 24, emphasis added.)

They go on to describe the Postal Service's method of measuring volume-variable costs as an "economically sound" approach, suitable for rate-making:

> The procedures adopted by the Postal Service of estimating forward-looking economic costs based on extrapolating the results of activity-based causal models of cost attribution is an economically sound starting point for identifying economic costs necessary for rate-making. (Id. at 27.)

They also are critical of assuming that mail processing costs are fully volume variable:

> The Docket No. R94-1 assumption of 100% volume variability for mail processing costs can be traced to Docket No. R71-1 documentation that is based on an analysis of 1953 to 1969 manual operations data. *It is more accurate to actively measure and calculate these elasticities than to continue to assume a 100% variability factor for all mail processing activities.* (*Summary Report* at 40, emphasis added.)[6]

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[6] As I described in my direct testimony, the 100 percent variability assumption has an even more tenuous link to statistical analysis than the Data Quality Study's authors suggest. USPS-T-15 at 128-130. Rather, the statistical analysis

While the authors of the Study note that criticisms of the cost driver/distribution key approach have been raised by intervenors in rate case testimony, they argue that the criticisms "pertain less to the theoretical structure of the Postal Service approach and more to issues of...implementation." Id. at 28, footnote 32. At no point in their study do they suggest that continuing to assume 100 percent volume variabilities for mail processing cost pools would be preferred to measuring the actual elasticities within each MODS cost pool.

The Data Quality Study also strongly supports the use of disaggregated cost pools in measuring volume-variable costs, since this corresponds to the theory of activity-based costing:

> The Postal Service uses an economically sound approach grounded in activity based concepts to determine its sub-class unit volume variable costs (UVVCs) on which Postal Rates are based. The categories of data collected and analyzed are sufficiently detailed and appropriate to arrive at the sub-class UVVCs. Id. at 32.

> As stated previously, the Study team believes the move to using MODS operational activity cost pools for mail processing costs is appropriate given the vast changes in mail processing operations over the past three decades. Id. at 123.

In short, the MODS cost pool approach is economically sound and an appropriate framework to deal with the "vast" and ongoing changes that have occurred in the organization of mail processing operations. Dr. Neels's criticisms of the cost pool/distribution key approach are empty and should be rejected.

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simply convinced the Postal Service's researchers to reject aggregate time series analyses – such as Dr. Neels's – as a basis for volume-variability.

VI. Dr. Neels's and Dr. Smith's criticisms of piece handling data for the manual operations are inapplicable to other MODS sorting operations.

MODS employs three distinct methods to measure piece handlings in sorting operations. For mechanized and automated operations, piece handlings are obtained directly from machine counts. Manual flat and letter piece handlings are derived from weight conversions and "downflows" from other operations. In manual parcel and Priority Mail operations, piece handlings are derived from manual piece counts and container conversions. Consequently, the quality of the MODS piece handlings data cannot be depicted with a broad brush. This has not stopped Drs. Neels and Smith, and Dr. Neels in particular, from attempting to cast doubt on the validity of the entire data set by focusing on a few allegedly egregious examples of data errors.

Dr. Neels, in particular, expended a great deal of effort seeking out possible reporting errors in the MODS piece handlings data.[7] Tr. 27/12797-12800. Neels would have us believe that each of these instances, which he documents in his Table 5 (Tr. 27/12799), is a data recording error. In fact, as I have stated previously, this is not necessarily correct.[8] Furthermore, he makes two subsequent arguments that are entirely unwarranted. First, he attempts to extend these alleged errors beyond the bounds of these two cost pools to other MODS operations. And second, he argues that the alleged errors he identified

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[7] See also Dr. Smith's comments at Tr. 27/13173.

[8] In response to oral cross-examination by UPS counsel, I indicated that the presence of a number of allegedly "suspicious" data gaps had far more prosaic and reasonable explanations. Tr. 15/6432-6436.

necessarily cause downward bias in my volume variability estimates. As I will show below, both arguments are specious and should be rejected.

As an example, let's consider the case to which he devoted the most space in his direct testimony, namely the one-year gap in piece handlings data for the Manual Parcels MODS operation group at a single site. Neels identified "positive piece handlings for Manual Parcels from the first quarter of 1993 to the first quarter of 1994, zero piece handlings from the second quarter of 1994 to the second quarter of 1995, and then positive piece handlings again." Tr. 27/12797-12798. For the sake of argument, suppose that he were entirely correct in saying that the zero TPF values for one year at site #6 all represent data recording errors. Even so, his analysis of this "error" is faulty.

First, Dr. Neels attempts to extend the presence of these alleged errors beyond the bounds of the Manual Parcels MODS cost pool to the SPBS cost pool. But there is no evidence on the record about data measurement errors in the SPBS piece handlings data, other than Neels's unsupported statements. To make his case, Dr. Neels concocted a theory that "Dr. Bozzo indicate[d] that the gaps in the data series correspond to periods where the *data* for the SPBS and Manual Parcels MODS activities were commingled and reported together as data for the SPBS MODS group." Tr. 27/12798 (emphasis added). This is false and misrepresents my comments. In oral cross-examination, I stated "that site [#6] *had handled manual and SPBS parcels together* up to a point prior to separating them according to the mail processing technology that was used to sort them." Tr. 15/6431. In other words, the commingling in question at site #6 represented

the physical commingling of parcels on site during sorting operations, not the commingling of data after the fact.

Even after Dr. Neels had had the error of his theory pointed out to him several times, he continued to cling to it uncritically. Response to USPS/UPS-T1-9, 45 at Tr. 27/12917, 13001-2. The reason for his tenacity seems clear: Dr. Neels doubtless believes that if he can convince the Commission that MODS data collectors are mixing together piece handlings data from manual cost pools with piece handlings data from automated and mechanized operations, then he can cast doubt on all of the MODS data—manual, mechanized, and automated—rather than only a single manual MODS operation at a single site.

Dr. Neels's theory that the SPBS and manual parcel piece handlings were "commingled" at site #6 (or elsewhere) is, quite simply, incorrect and inconsistent with MODS data collection procedures. SPBS is a mechanized sorting operation, and as with other mechanized and automated sorting operations, SPBS piece handlings are obtained from machine counts. Since a piece has to be handled on the SPBS to be counted in SPBS TPF, there is no way for pieces handled manually to enter the SPBS TPF count. By contrast, manual parcels (and Priority) volumes are manually logged. Tr. 15/6387. In fact, after many interrogatories and responses, Dr. Neels has conceded as much. Response to USPS/UPS-T1-45 at Tr. 27/13001.

Dr. Neels's second line of argument—that the presence of these alleged errors in Manual Parcels TPF led ineluctably to a downward bias in my econometric volume variability estimate for that cost pool because of

measurement error, is even more easily dismissed. One can do so in either one of two ways. The first is to refer to the relevant portions of Dr. Greene's testimony, in which he disproves the argument on theoretical grounds. USPS-RT-7 at 21-26. The second is even simpler: in making this argument, Dr. Neels is conveniently choosing to ignore my comments (Tr. 15/6388) indicating "that the manual parcels observations from this site [#6] do not enter the manual parcel regression sample," which makes this specific complaint completely irrelevant to my econometric results.

VII. General appraisal of Dr. Smith's testimony

In my direct testimony, I gave Dr. Smith substantial credit for his observations in Docket No. R97-1 that some aspects of Dr. Bradley's mail processing "cost equations" may have been inconsistent with standard economic cost theory. USPS–T–15 at 31. Addressing Dr. Smith's concerns motivated, in whole or in part, a number of important elements of my analysis, particularly the inclusion of additional variables in the models to ensure consistency with the applicable economic theory.

In his current testimony, Dr. Smith has manufactured a list of "fatal flaws" in my analysis as extensive, if not more, than his objections to Dr. Bradley's analysis. However, his objections to my study are devoid of substance. He offers nothing more than a convoluted mass of cosmetic gripes, misinterpretation of the testimony of several Postal Service witnesses (including myself), statistical

errors, faulty and self-contradictory (and sometimes flatly absurd) theoretical prescriptions, and—since Dr. Smith conducted no independent quantitative analysis of my data or models (see Response to USPS/OCA-T4-9, Tr. 27/13249)—entirely unfounded quantitative speculation about my econometric results. A summary of Dr. Smith's major arguments and the rebuttal follows.

VII.a. Cosmetic Gripes

A number of Dr. Smith's criticisms of my analysis are purely cosmetic, and therefore do not impeach my analysis. Dr. Smith objects to my interpretation of the variability models as "labor demand functions" (as opposed to Dr. Bradley's term of "cost equations"), claims I failed to provide the theoretical derivation of the models, and asserts that my presentation of the facility capital variable is unintelligible. Tr. 27/13167-8, 13180. The complaints are trivial and poorly founded.

Dr. Smith's claim that "we are faced with... cost functions that have become labor demand functions" (Tr. 27/13217-8) incorrectly characterizes both my testimony and Dr. Bradley's. Dr. Bradley garnered some criticism by calling his models "cost equations," which he specifically distinguished from cost functions. USPS-T-15 at 42. I maintain throughout my testimony that my regression models represent labor demand functions; the same would be an appropriate clarification of Dr. Bradley's "cost equation" terminology. There is no metamorphosis of the functions being estimated.

In this case, Dr. Smith argues not that the labor demand functions I estimate are inconsistent with economic cost theory, but rather that I did not explicitly perform the derivations. Tr. 27/13187. Ironically, Dr. Smith cites my response to OCA/USPS-T15-56, in which I explain (verbally) the economic motivation for my models. In that response (at Tr. 15/6358), I note that none other than Dr. Smith confirmed in Docket No. R97-1 the mathematical substance of the derivation of conditional factor demand functions from the cost function. Docket No. R97-1, Tr. 28/15909-10. In short, the mathematical foundation Dr. Smith contends was lacking had already entered the evidentiary record of the Docket No. R97-1 proceeding. To the extent I introduced new concepts, I provided detailed citations to authoritative sources in the economics literature. Dr. Smith does not claim that the derivation cannot be performed (Response to USPS/OCA-T4-7 at Tr. 27/13246) nor does he provide an alternative derivation that demonstrates any error. This critique is consequently without substance.

Dr. Smith's complaint that my "testimony does not discuss QICAP" (Tr. 27/13196-7) is true only in the narrowest of senses—QICAP, the TSP variable name for my facility capital index, indeed does not appear in the text of USPS-T-15. However, I did discuss its data sources and inclusion in the labor demand models. USPS-T-15 at 93-94, 116. I also responded to numerous interrogatories from the OCA and UPS investigating the foundations of the variable. In fact, Dr. Neels was able to use the information I provided to demonstrate the deployment of various types of equipment over the period of time covered by my sample. Tr. 27/12780. Dr. Smith is able to extract such

detailed information about the derivative of the capital index as the depreciation rates by asset category. Tr. 27/13182. Since there is only one facility capital index used in the study, there is no real ambiguity.

VII.b. Misinterpretation of Postal Service testimony.

Dr. Smith bases his contention, that I potentially erred in not using a simultaneous equations estimator to reflect the endogenous nature of capital, on a string of misinterpretations of my testimony as well as those of witnesses Degen (USPS-T-16) and Kingsley (USPS-T-10). Dr. Smith's contentions, that "it is not clear whether capital is an exogenous or endogenous variable" (Tr. 27/13168) and that I indicate "that capital is neither exogenous nor endogenous" (Tr. 27/13201), misrepresent my testimony. I explained that I treated capital as "predetermined." Tr. 15/6414. This term reflects the fact that the investment decisions that determine current period capital occur well in the past, as well as explaining my choice of estimation procedure. In econometrics, "predetermined" variables include exogenous and lagged endogenous variables—the term is used in virtually every textbook treatment of the simultaneous equations problem, including those cited in his response to USPS/OCA-T4-21 (see Tr. 27/13268-9). The significance of the term is that a simultaneous equations estimator is not needed for a regression in which all of the explanatory variables are predetermined. The terminology I used should have clarified my treatment of capital to Dr. Smith.

Dr. Smith attempts to take issue with my characterization of capital as predetermined on operational grounds as well. He states, without citations, "Based on information furnished by the Postal Service, it appears that the current level of capital is related to the current level of activity, though not necessarily on a 100 percent basis." Response to USPS/OCA–T4–21(d) at Tr. 27/13269. Asked to provide supporting citations to the referenced Postal Service information in USPS/OCA-T4-51 (Tr. 27/13310), Dr. Smith cites two of my interrogatory responses, portions of witness Degen's and witness Kingsley's testimonies, and the Postal Serivce's 1999 Comprehensive Statement on Postal Operations. The material he cites does not support his characterization of capital costs. For example, he cites my response to OCA/USPS-T15-14, which does not concern capital costs at all. My response to OCA/USPS-T15-13, also cited, indicates that major equipment deployments usually take more than one year. Witness Degen's cited testimony, emphasizes that

> One reason for this deliberate pace [of new plant construction] is the enormous time and capital commitments involved. From initial proposal to project completion, it may take anywhere from 6 to 9 years to bring a new plant on line. Site acquisition, planning, and approval for a new plant can easily take 5–7 years, and actual construction another 1–2 years. USPS-T-16 at 15.

Likewise, a cited section of witness Kingsley's testimony indicates that the initial phase of AFSM 100 deployment was scheduled to begin in March 2000, with a second phase deployment planned to begin at the end of FY 2001. USPS-T-10 at 11. These responses make it clear that there are long lead times between investment decisions and the appearance of new plants and capital

equipment on the workroom floor. The conclusion Dr. Smith draws from the cited material is virtually the opposite of its plain meaning. The cited material supports my treatment of capital as predetermined.

VII.c. Statistical errors.

In USPS-RT-7, Dr. Greene describes several fundamental statistical errors Dr. Smith commits in his testimony, including the erroneous claim that the between model is the "least bad" among the alternative estimators, and the faulty suggestion that visual analysis is a "compelling" substitute for an appropriate quantitative study. USPS–RT–7 at 31, 37-8. Dr. Smith himself admits that the simple regression analysis corresponding to the visual exercise is "econometrically indefensible." Tr. 27/13215. Dr. Smith's erroneous econometric prescriptions must be rejected. His contention that I could have potentially increased the accuracy of my estimates by considering clusters of sites in lieu of the panel data estimation approach (Tr. 27/13174) is also faulty. A clustering approach would have constituted another type of aggregation procedure. Once again, if aggregation were appropriate, the disaggregated models would produce results consistent with the aggregates. The clustering procedure cannot add information to the variability analysis, but rather only create the potential for bias from imposing inappropriate restrictions on the variability models. Dr. Smith's erroneous econometric prescriptions must be rejected.

VII.d. Faulty and self-contradictory theoretical positions

Dr. Smith's testimony relies on a number arguments that are transparently self-contradictory. Chief among these is Dr. Smith's inconsistent position on the fundamental issue of whether multiple regression analysis is required for the variability study. As Dr. Greene indicates,

> It is clear that it is appropriate to use multiple regression to model the response of labor costs to output—the appropriate definitions of these two variables and how to measure them is an issue to be settled elsewhere. A simple regression of hours (or its logarithm) on output of any sort (or its logarithm) will surely ignore many other factors that that should be in the equation... USPS-RT-7 at 6.

Some of Dr. Smith's criticisms imply that there are additional variables that I should have included in my models but did not. For example, he claims that "Capacity utilization is another potentially important variable missing from Dr. Bozzo's database." Tr. 27/13184.[9] For Dr. Smith's statement to have any practical meaning for the labor demand models, it would have to be that capacity utilization should be added as an explanatory variable to the models. This would make the appropriate model a multivariate regression *a fortiori*. On one hand he suggests that I do not have enough variables in my model, but on the other hand he is unsure whether a multiple regression model is appropriate. Dr. Smith's response to the question of whether a multivariate regression model is

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[9] Dr. Smith's statement is, in itself, erroneous. The capital and labor data needed to compute measures of capital (i.e., "capacity") utilization are present in the database. Furthermore, since workhours are endogenous to the models, capital utilization is implicitly determined by the models as well.

appropriate is "I don't know." Response to USPS/OCA-T4-16(a) at Tr. 27/13262. By way of explanation, Dr. Smith offers:

> Two important variables for the analysis of volume variability appear to be TPH and hours. On a bivariate basis they seem to be closely associated. Applying the concept from William of Ockham, *Pluralitas non est ponenda sine necessitate* (this translates as "entities should not be multiplied unnecessarily." Put differently, "keep it simple"), also known as Ockham's Razor, one would look for the simplest explanation, and a simple explanation is that there is a very high degree of relationship between the two variables: it is visually compelling. Id.

One wonders if the only reason why he is unable to say whether a multivariate model is appropriate is because he is unable to figure out how an appropriate multivariate model can be made to produce the 100 percent variability result. In contrast, results from the simple regression model, such as those Dr. Smith presents at page 66 of OCA-T-4, more-or-less do.[10] The catch is that the bivariate models are "econometrically indefensible." Tr. 27/13215. All Dr. Smith can offer is a paean to simplicity—hence the invocation of the maxim of "Ockham's Razor."

Ockham's Razor, however, does not value simplicity at any cost—this is the vital "unnecessarily" in the direct translation. This maxim, as Carl Sagan nicely puts it, "urges us when faced with two hypotheses that explain the data *equally well* to choose the simpler." (Carl Sagan, The Demon-Haunted World, New York: Ballantine Books, 1996, at page 211;

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[10] However, note that Dr. Smith's results show a 19 percent "variability" for the "OCS [sic]" operation—presumably this means OCR. To be consistent, Dr. Smith would have to maintain that there is "visually compelling" evidence that OCR costs are 19 percent volume-variable.

emphasis in original). The bivariate models are definitely simpler, but they do not explain the data as well as the multivariate models. The specification tests that favor the more complicated multivariate models tell us loudly and clearly that the additional complications are necessary. Rather than draw the correct conclusion that the bivariate models are biased, Dr. Smith concludes that the multivariate models must somehow be wrong.

Dr. Smith's testimony incorprates inconsistencies on points of economic theory as well. The Intriligator work he cites in support of his "expansion path" arguments (discussed in more detail below), motivates the "expansion path" in the context of profit maximization.[11] Response to USPS/OCA-T4-2 at Tr. 27/13240-1. However, he goes to some length to argue that the Postal Service is actually an "output maximizer" a la Soviet manufacturing industries. OCA-T-4 at 47, 49. The objectives of profit and output maximization are inconsistent, since "output maximization" would tend to require unprofitable behavior such as selling product below cost. In fact, neither of the behavioral models Dr. Smith offers is particularly applicable to the Postal Service. The Postal Service's statutory break-even requirement interferes with profit maximization, while the requirement that prices at least cover "attributable" costs, among other things, makes output maximization difficult. Its inability to freely choose its prices limits both types of behavior. Indeed, Dr. Smith's "evidence" in support of the output

[11] Since, as I discuss below, the "expansion path" and cost function are conceptually identical, the "expansion path" does not depend on profit maximization for its existence.

maximization hypothesis is extraordinarily thin, consisting primarily of a reference to a speech in which a Postal Service vice president emphasizes the importance of revenue growth. Response to USPS/OCA-T4-13(c) at Tr. 27/13257. It should be transparently evident that the Postal Service operates in an environment dramatically different from Soviet enterprises, and is, in various ways, prevented from exhibiting output maximizing behavior. All Dr. Smith has done in his output maximization argument is to follow a far-fetched claim to its logical but absurd conclusion. He does not provide a useful characterization of the economic framework for mail processing costs.

VII.e. Unsupported allegations.

Dr. Smith makes a number of allegations that my estimates are potentially sensitive to a variety of factors, including structural changes to Postal Service operations and the sample selection procedures. Tr. 27/13169-77. As Dr. Smith performed no analysis of his own (Tr. 27/13249), he offers no evidence in support of the allegations. In fact, in many cases, he simply ignores responsive analysis I presented in my direct testimony. In USPS–T–15, Appendices A and B, I present alternative variability estimates varying the minimum observations screen and dispensing with all of the sample selection screens entirely. The results clearly show that, contrary to Dr. Smith's allegation, the presence of the sample selection screens do not drive my results. Nor did I ignore the issue that the earlier years' data may not be fully representative of future operations. Thus, in Appendix D of USPS–T–15, I presented the results of alternative variability calculations in which only FY98 observations were used to evaluate the elasticity

formulas. Once again, the results are robust to the period over which they are evaluated. Dr. Smith's concerns are not merely groundless, they are contradicted by evidence already on the record in this proceeding.

VII.f. Dr. Smith's "erratum" revising the definition of volume variability introduces an error into Dr. Smith's testimony.

As I demonstrate in this section, the revision of the definition of volume variability in Dr. Smith's erratum to his direct testimony not only introduces an error and contradiction into that testimony, but calls into question Dr. Smith's basic understanding of econometric model construction and interpretation.

Dr. Smith's initial direct testimony correctly defines "[v]olume variability for mail processing...as the percentage change in cost that results from a [unit] percentage change in volume, *holding delivery points and other non-volume factors constant.*" Tr. 27/13153. In a subsequent section of his testimony Smith expounds on the importance of including measures of network effects, including possible delivery points, in the analysis of mail processing variability, noting the possible presence of "three types of network issues" in modeling mail processing labor demand:

> First, there is the intra-plant network of activities that feed mail to each other....A second type of network effect is apparently the delivery configuration of the service territory. Dr. Bozzo measures this network configuration with a variable measuring the number of possible deliveries [in the plant's service territory]. Finally, the position of the plant in the mail flow between other mail processing plants also seems to be a type of network relationship. According to an interrogatory response, the size of facilities and their mail processing operations depends not only on the volume of mail processed, but also their position in the Postal Service's network. Id. at 44 (footnote omitted).

In his testimony, Dr. Smith emphasizes the importance of network effects in models of mail processing labor costs, citing their importance in determining, among other things, "the length of processing windows, the complexity of mail processing schemes, the relative amount of labor required for set up and take down activities, [and] the operation's role as a gateway or backstop." Id. at 45. Indeed, he even expresses concern that my models may have included *fewer* than the optimal number of controls for the various types of network effects:

> The analysis conducted by Dr. Bozzo addressed only the possible deliveries; he did not address the networking of activities at the plant level or the interchange of mail between plants. Both of these types of network effects might have an impact on labor demand. Id. (footnote omitted).

I was therefore puzzled when, over a month after filing his direct testimony, Dr. Smith appeared to have inexplicably changed his mind about the importance of including measures of network effects in the regression. In a revision to Smith's direct testimony labeled "Erratum," the phrase "holding delivery points and other non-volume factors constant" was stricken from the sentence on page 5 cited above.[12] The erratum stated that the deletion was necessary "to eliminate an inappropriate restriction on the volume variability definition as previously indicated in witness Smith's response to USPS/OCA-T4-11(b) and to eliminate any uncertainty as evidenced by...interrogatories USPS/OCA-T4-33 and 34(b)." This was accompanied by Smith's responses to USPS/OCA-T4-33 and 34, which note the need "to remove a statement in my

[12] "Revision to the Testimony of witness J. Edward Smith (OCA-T4)(Erratum)" filed June 28, 2000.

direct testimony that conditioned the definition of volume variability upon holding delivery points and other non-volume variables constant." Tr. 27/13284-5.

All the more puzzling is the fact that while striking this clause, Dr. Smith neglects to remove the above-cited material from pages 44 and 45 of his direct testimony extolling the importance of network effects in models of mail processing labor costs. Tr. 27/13193-4. The net effect of this that Dr. Smith's direct testimony (as amended) is in direct conflict with itself, on one hand asserting that network effects are key elements of the analysis, and on the other insisting that the econometric estimates of the variabilities should not be conditioned on them.

The key to explaining this confusion in Dr. Smith's testimony is evident from a close reading of his responses to USPS/OCA-T4-11 and 34. In his response to 11(b) witness Smith claims that:

> [i]n computing the volume variability, Dr. Bozzo...estimated the multivariate econometric model of hours of labor as a function of TPF and other variables; only the estimator associated with the TPF variable is used in computing the variability. *Accordingly, in order to be precise, the statement should be "the percentage change in cost that results from a [unit] percentage change in volume"* (emphasis added). Tr. 27/13254.

In comparison, witness Smith states in his response to 34(a):

> On further review, it is apparent that Dr. Bozzo has used more than the estimator associated with the TPF variable in computing [the] variability. The appropriate annotation is found in footnote 36 at 76 in Dr. Bozzo's testimony. *I believe it was Dr. Bradley who used only the estimator associated with the TPF* [sic] *variable in computing [the] variability* (emphasis added). Tr. 27/13285.

The apparent problem is Dr. Smith's mischaracterizations of the derivation of the variabilities at Tr. 27/13254 and 13285. The variabilities, in both my study and Dr. Bradley's, are appropriately computed as the partial derivative of the labor demand function with respect to TPH. The resulting formula depends on TPH and the other variables in the labor demand model. Dr. Smith states that since I do not include the "estimator" associated with delivery points in my computation of the variability factor, it would not be "precise" to say that delivery points had been held constant. Tr. 27/13254. Dr. Smith is wrong on this point, as may be verified by examining any econometrics textbook. The correct computation of volume variability (as provided in USPS–T–15) must hold constant (or be "net of") delivery points and the effects of other non-volume factors, otherwise we would not have proper measures of volume variability, but rather a confounding of volume and non-volume effects. One does so by including delivery points and other non-volume factors in the regression model. This does not imply that one should include the coefficients corresponding to these factors explicitly in the variability formula. As I mention above and in my direct testimony, this was well known as of Docket No. R71-1. Dr. Smith's "erratum" obscures, rather than clarifies, the correct definition of volume-variability.

VII.g. The Postal Service's cost methods, taken as a whole, embody the correct "length of run"—which is not the "long run" advocated by Dr. Smith.

Dr. Smith incorrectly claims that the Postal Service's mail processing cost analysis is "fatally flawed" because it is not a "long run" analysis. Tr. 27/13167 et seq. His criticism is hardly new, but unfortunately it has not improved with age. In Docket No. R97-1, Smith claimed that the high frequency of Dr. Bradley's data —i.e., observations every postal accounting period—combined with the use of the fixed-effects model, caused Bradley's variability estimates to be inappropriately "short run." Docket No. R97-1, Tr. 28/15835-41. As I note in my direct testimony, Dr. Smith's arguments about length of run in the previous rate case were specious and without merit, and were successfully rebutted in the record evidence of that case.[13] In the present docket, Dr. Smith makes a similar claim, but has largely backed away from the arguments he proffered last time. Instead, he erroneously asserts that nothing but a "long-run" analysis – by which he means one in which all factors of production, including plant and equipment, are assumed to vary freely – will do for purposes of ratemaking. Tr. 27/13189.

Once again, Dr. Smith is wrong. He claims without substantiation that:

> Postal Service witnesses and management appear to have a time frame of as little as one year to as many as five years in mind when they discuss the longer run, the period over which capital investment varies. The time frame seems to center on two to three years. Tr. 27/13190.

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[13] See USPS-T-15 at 18, lines 16-19, which cites the rebuttals by witnesses Higgins and Bradley to this line of argument in Docket No. R97-1. See also id. at 71-72.

He goes on to state:

> [I]t would appear that there are several time periods relevant to the estimation of postal costs. One time period is a day, the period over which very short-term adjustments to labor are made....A second time frame appears to be the 4 week or 3 month time frame used by Dr. Bradley and Dr. Bozzo....Finally, a longer-run time period, which would appear to approximate the length of the rate effective time period in the neighborhood of two years, seems to be the time frame over which investment, personnel, and equipment decisions are realized. Tr. 27/13191.

Only the first of these "time frames" has any basis in the record evidence of this case. As I testified, the process of assigning the existing labor complement in a plant to various operations to meet immediate processing needs does, indeed, operate "on time scales on the order of hours." USPS-T-15 at 18. This comports with Smith's first "time frame" of a day. Smith's second reference, to "the 4 week or 3 month time frame" used by Bradley and myself refers not to any operational decision-making framework, but rather to the frequency of our data (accounting periods and quarters, respectively). Contrary to Smith's understanding, the periodicity of the data used to analyze costs does not determine the length of run of the analysis. As I have already discussed, that particular argument was rebutted in the previous rate case and should carry no weight.

Dr. Smith's final reference in the above-cited passage, to the "longer-run time period, which...[is] in the neighborhood of two years" and "over which investment, personnel, and equipment decisions are made," has no basis in fact. This is mere conjecture—he cites neither record evidence nor any authority versed in the subject of management decision making. As I have already testified, management decisions concerning long-run labor allocation and

investment are independent of the "rate cycle." USPS-T-15 at 18. Since models of labor demand of the sort I developed in my analysis are properly based on the actual planning practices of actual line managers, rather than abstract theorizing, there is no basis for incorporating Dr. Smith's third "time frame."

Having posited, without evidence, that only a "long-run" model is appropriate for ratemaking, Dr. Smith attempts to discredit my inclusion of a plant-level capital index in the labor demand function. As Dr. Smith notes, I freely admit that my volume-variability estimates are "short run" in the sense of treating capital as a quasi-fixed factor. Tr. 27/13190. I fully intended to do so: my treatment of capital as quasi-fixed is reasonable and comports with the mainstream of econometric cost analysis.[14] My treatment of capital does not mean to imply that my variability estimates assume that the Postal Service never changes its capital stock, or that no new net investment takes place[15]. Nor does it mean, as Smith asserts, that my estimates are "only measuring transitory changes in mail processing." Tr. 27/13190. Quite the contrary: my model incorporates an explicit measure of capital into the model, along with a time trend, to allow for continuous changes to the capital stock, and with it the level of

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[14] For a general discussion, see chapter 9 of Ernst R. Berndt, *The Practice of Econometrics: Classic and Contemporary*, Addison-Wesley 1991. For an application, see Douglas W. Caves, Laurits R. Christensen, and Joseph A. Swanson, "Productivity Growth, Scale Economies, and Capacity Utilization in U.S. Railroads, 1955-1974," *American Economic Review* Vol. 71, No. 5 (December 1981), 994-1002.

[15] Dr. Smith's confusion on this matter may be related to his misunderstanding of the term "to hold constant," as I discuss in Section XIIIa.

technology.[16] I have included an index of the plant's net capital stock in my regression model, so that my estimate of the volume variability of labor hours in an operation is conditional on the level of capital in place in the current period.

That Dr. Smith should criticize my analysis for including a capital measure is ironic, given that in his R97-1 testimony Dr. Smith criticized Dr. Bradley for failing to include such a measure:

> In my opinion, witness Bradley's translog cost equation is insufficient, for he does not include capital as one of the cost factors. Witness Bradley needs to examine the underlying production function and cost function and the derivation of the cost function. He also needs to examine capital/labor substitutions, scale economies, and the interrelationships of activity processes in conjunction with his estimated cost equation. This will enable an understanding of the impact of changes in capital and technology on the cost in labor hours as TPH varies during mail processing. Docket No. R97-1, Tr. 28/15828. See also Tr. 28/15823, 15825, 15826-7, 15850-52.

In Frank Capra's classic movie, "It's A Wonderful Life," the angel Clarence warns Jimmy Stewart's character, George Bailey, "Be careful what you ask for, George – you might get it!" Dr. Smith could learn a thing or two from Clarence. In the previous rate case, Smith "asked for" a regression model that included, among other things, a measure of capital. Now he has what he asked for, but it has not apparently made the kind of difference to the results that he anticipated. Smith has turned his old argument on its head and tried to use it as a basis for shoring up his previous argument about length of run, which was successfully rebutted in Docket No. R97-1. That is, whereas (according to Dr. Smith) in the

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[16] See Dr. Greene's rebuttal testimony (USPS–RT–7) at 11-13, 31-34 for further discussion of this point.

last case Dr. Bradley's model was no good because it didn't contain a capital variable, in this case my model is excessively "short run" because it does contain a capital variable. At best this is disingenuous. An econometric model should be specified based on economic theory, not on whether the results fit one's own purpose.

VII.h. The theoretical foundations of the Postal Service's mail processing labor demand models and of Dr. Smith's recommended "expansion path" approach are identical

The Postal Service's mail processing labor demand analysis is consistent with Dr. Smith's "expansion path" approach, which he claims is the conceptually correct economic relationship to estimate. Tr. 27/13167.

Dr. Smith himself establishes that the expansion path argument does not constitute a criticism of the Postal Service's variability methods at all, for the simple reason that the cost function and expansion path are conceptually identical. Citing several authoritative texts, Dr. Smith explains, "the set of all possible pairs of output and cost along the expansion path define the cost curve." Tr. 27/13267. He further notes that "[i]n general, one can obtain a system of factor demand functions" derived from the expansion path or cost function. Id. He also confirmed that the short-run cost function simply represents an alternative expansion path, and that the long-run cost function must be below the short-run cost function for every level of output. Tr. 27/13304.

Dr. Smith also confirmed in part the substance of a number of statements, including derivations of the relationship between the expansion path and the

elasticities Dr. Bradley and I estimated. Tr. 27/13304, 13323. It follows immediately from the theory that Dr. Smith cites that the degree of volume-variability along the expansion path is the elasticity of labor demand (workhours) with respect to output. These are precisely the quantities Dr. Bradley and I estimated.

VIII. Conclusion

My review shows that Drs. Neels and Smith have provided no credible basis to challenge the conclusions presented in my direct testimony. Their attempts to sustain the general assumption of 100 percent volume variability for mail processing do not withstand scrutiny.